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No. 8.

Admiral MANNERS, President, in the Chair.

W. W. Magee, Esq., University College, London,

was balloted for and duly elected a Fellow of the Society.

Note on the Transit of Venus in 1874.

By J. R. Hind, Esq., F.R.S.

In the *Comptes Rendus* for 1861, July 22, will be found the results of a careful calculation by myself of the circumstances of the transits of 1874 and 1882 derived from M. Le Verrier's Tables of the Sun and Planet. Of the accuracy of the Right Ascensions and Declinations which I employed there can remain no doubt whatever; they have not only been confirmed by a new computation conducted in the *Nautical Almanac* Office, but the Astronomer Royal has since obtained identical results. I was, therefore, somewhat surprised to find that the times of contacts of the centre of *Venus* with the Sun's limb, given by M. Puiseux in the Appendix to the *Connaissance des Temps* for 1871, differed sensibly from the time I assigned in the *Comptes Rendus*, the same tables having been employed in both calculations. I had not much faith in the efficacy of a re-examination by myself of my own calculations, but Mr. Bishop having allowed me to place the matter in the hands of his assistant, Mr. W. Plummer, a very able and accurate computer, I am enabled to state that my published numbers are nearly correct, and that errors must have

crept into M. Puiseux's work. The times deduced by Mr. Plummer are—

	Ingress.			Difference	
	G.M.T.			Plummer —	Hind.
	h	m	s	s	
External contact, December 8th	13	46	42	—	14
Internal contact, „	14	15	30	—	27

Egress.					
Internal contact, December 8th	17	57	8	+	3
External contact, „	18	26	6	+	1

These differences for such a phenomenon are insignificant. The possible errors of any predictions of the times of contact must be very much larger.

1869, June 10.

The Transit of Venus in 1874. By R. A. Proctor, B.A.

In the *Monthly Notices* for March last, I indicated my intention of applying more exact modes of examination to the circumstances of the approaching transits of *Venus*, than had hitherto been considered necessary. In resuming the examination of the transit of 1874, I feel it necessary to call attention to that statement of my object, lest it should be thought that the corrections which I have been led to make on former results, point to errors in the processes by which those results were obtained, whereas, in reality, they are only due to the intentional neglect in prior work of considerations which even now many may be disposed to look upon as unnecessarily exact for the purposes of a preliminary inquiry. To still further obviate this objection (which, however, is not likely, I think, to occur to any one who has read my former papers), I shall for the most part refer to former results as those which have been obtained on such and such suppositions. This is the more advisable, because I find that astronomers are willing to accord the same degree of accuracy to my results in relation to the modes by which they have been obtained, as I am ready to assign to others results with a corresponding *proviso*.

It may be necessary, however, before proceeding to a detailed examination of the circumstances of the coming transit, to inquire (1) how far it is necessary or advisable to aim at exactness in the preliminary investigation of the subject; and (2) whether the phase which I have selected for examination is well chosen.

As a reason for applying a comparatively rough mode of

examination to the circumstances of the transit, it has been urged that there is some uncertainty as to the exact moment of conjunction of *Venus* and the Sun, and as to the exact distance which will separate their centres. What is the range of possible error due to this cause I will leave to others to determine. My own impression is, that it is very small, whether considered absolutely or with reference to other sources of error which it is in our power to remove. But whether this be so or not, it seems clear to me that the possible errors, as to time and position-angle, arising from this cause, must be looked upon as affording additional reason for exactness in other matters. For, should any error resulting from disregard of exact considerations happen to be *additive* to errors resulting from inaccuracy in the Planetary Tables, it might happen that the conclusions to which we should be drawn would affect appreciably the success of observations to be made upon the transit.

The possible inaccuracy of the Planetary Tables seems to be the only source of error which it is out of our power to get rid of altogether. Therefore, as the choice of stations depends in certain cases on considerations of some nicety, it seems to me that the only basis for a proper selection is the determination, with as much accuracy as is readily attainable, of the circumstances under which the phase selected for observation will be seen at different stations. It must be remarked, however, that the formulæ and calculations by which I have determined these circumstances are not by any means necessary. A process which I will presently indicate suffices to give, with scarcely any labour, all the necessary information respecting a large number of stations, and that with a degree of accuracy amply sufficient for all the purposes of the inquiry. The only way, however, in which I could conveniently present my results for the study of those who take interest in the subject of this transit was that which I have selected.

As to the selection of internal contacts as the phase to be specially dealt with, there may be, of course, a variety of opinion. I am told, for instance, that in France there is some question of observing external contacts also. In this case, it would be advisable to make external contacts the determining phase (if a choice had to be made), or to determine for several stations the circumstances of both phases. For as at ingress external contact takes place about half an hour before internal contact (for every station), it is clear that the stations for observing retarded ingress (always so selected that the Sun is lately risen) must be chosen with reference to external contacts, if it is a *sine quâ non* that these are to be observed. If such a station were selected with reference to internal contact, with a moderately good solar elevation (necessarily *soon after* Sunrise), it is obvious that at the epoch of external contact half an hour earlier, the Sun would be too low; and considerations of the same sort apply to the observations of accelerated egress.

It appears to me for these reasons that the consideration of both phases is inadmissible, and that though external contacts may be observed with advantage when they happen to be visible, *as well as* internal contacts, yet the stations must be chosen with strict reference to the latter phase, which is admittedly far the better of the two.

As M. Puiseux has given reasons for selecting the passage of *Venus' centre* over the Sun's limb as the phase to be considered in the preliminary inquiry, it may be well to examine how far those reasons are valid. He remarks that what is actually observed is the exterior or interior contact, but that, as the epochs of these phases are separated by nearly constant intervals for all stations, we may neglect the consideration of those intervals when all that we require is to determine the relative values of different stations. I venture to submit the following reasons for looking upon this line of argument as inadmissible:—

It is to be noted, in the first place, that the interval between external and internal contact is not constant, and although its range (from $26^m\ 24^s$ to $31^m\ 18^s$) may seem small, yet, where the values of stations run close, the differences thus resulting are quite sufficient to turn the scale in several instances. But also we may look at the matter in this light: the change of phase results in a change in the position of the stations of maximum acceleration, or retardation, and this change amounts in certain cases to hundreds of miles. Now the circles which give a definite coefficient of parallax in one of these cases, have equal radii, but their poles (the stations just mentioned) are separated by a considerable distance. Therefore the circles must intersect. At a point of intersection the coefficients are equal for either phase: on one side of that point, the nominal value of the coefficient is improved by the change of phase; on the other, it is deteriorated. This proves that the relative values of stations cannot remain unaffected by a change of phase, and suffices to account for the corrections introduced by my consideration of internal contacts in place of the passages of *Venus' centre*.

It would be incorrect, however, not to mention that the larger part of the corrections I now propose to consider, results from another cause than the change of phase; namely, from the effects of parallax,—which it has not hitherto been thought necessary to take into account. The application of the correction due to the equation of time has also to be mentioned as appreciably affecting the extent of the changes which are here dealt with.

I may summarise these changes under three heads:—

1. *The application of Delisle's method of absolute time differences.* The relative as well as the absolute values of many stations are affected by the change of phase. Some which had hitherto appeared unsuitable are found to be unobjectionable. Others which seemed good appear unfit. In other cases the relative values of two stations are so affected that the results of a comparison between them are directly reversed. Lastly, many

stations not hitherto thought of in connexion with the transit are found to be well suited for the application of Delisle's method.

2. *The comparison between Delisle's and Halley's methods.* Halley's method is found, not merely to be applicable with advantage, which is all that can be said of it when central passages are considered, but to be superior to Delisle's,—slightly, when reference is made only to such stations as had been hitherto dealt with, noticeably when Antarctic stations are made use of.

3. *The comparison between the transits of 1874 and 1882 with reference to Halley's method.* This comparison, conducted according to the principles laid down by Mr. Stone (than whom no one is better entitled to pronounce authoritatively on such points) shows that Halley's mode may be applied much more advantageously to the transit of 1874 than to that of 1882.

I premise that I have carefully gone over all the calculations on which the results of my former paper were founded, and have applied so many independent modes of calculation that I can no longer entertain any doubt as to the accuracy of those results.*

The results to be now brought into comparison for the sake of forming an estimate of the effect of phase, parallax, and the equation of time, upon the values of various stations, may be thus classed:—

- A, those derived from the consideration of central passages, as supposed to be seen from the Earth's centre, with the position-angles corresponding to external contact.
- B, those derived from the same phase, similarly seen with the position-angles corresponding to central passage.
- C, those derived from the consideration of internal contacts, as seen from the stations themselves, and with the position-angles corresponding to the phase so seen.

Note.—The results under head C have alone been corrected for equation of time.

The following table exhibits the position-angles and epochs (for ingress) corresponding to A, B, and C:—

	Position-Angles.			Epochs.		
	A	B	C	A	B	C
	°	°	°	h m	h m	h m
Accelerated Ingress	131	133 29	133 56	14 0	13 55	14 4
Mean Ingress	131	133 29	136 28	14 0	13 55	14 16
Retarded Ingress	131	133 29	139 29	14 0	13 55	14 29

In preparing the columns under head C in the accompanying

* This paper had originally exhibited an independent mode of establishing my conclusions, founded on a simple construction applied to the elements given in Mr. De La Rue's paper on the transit (*Monthly Notices* for December 1868). For the sake of brevity, I have thought it best to omit this part of the paper, the accuracy of my conclusions not being, I find, called in question. The mode of construction is, however, briefly referred to further on.

tables, I have made use of six orthographic maps which accompany this paper. These were constructed with every precaution to insure accuracy. The intersection of longitude-lines and latitude-parallels (to every 10°) were separately constructed for by a double process, and in all critical cases further tests were applied. In all, the construction of the maps involved upwards of 3000 measurements, and from the scale on which the maps were constructed, the practice I have had in orthographic mapping, and the care used in the present instance to insure accuracy, I feel that I may safely claim for the numbers under the columns headed C in the following tables, a satisfactory degree of trustworthiness.

The six maps include four quarter-spheres, exhibiting the solar elevations and the coefficients of parallax in the same way as in the maps 1-4, in the *Monthly Notices* for December last, though in a different projection. The other two exhibit the Earth as supposed to be seen from the Sun at ingress and egress (*mean*, and for internal contacts). In these the solar elevations are indicated by circles, and in place of 10 parallactic parallels, corresponding to the parallactic circles in the other map, there are laid down parallactic lines corresponding to minute intervals (the line across the earth's centre being taken as a zero-line). These lines are not parallel, but separately constructed for. Thus their indications differ somewhat from those derived from the parallactic circles in the other maps, which are laid down on the supposition (not strictly correct) that the outline of the penumbra of *Venus* travels parallel to itself across the face of the earth. This will account for a slight want of correspondence between the second and third C-columns in the following tables: the third gives the correct effect of parallax. It will be noticed, however, that the difference is always trifling in the case of places suitable for the application of Delisle's method.

A further correction, but one of small importance, would result from the consideration that the apparent outline (supposed to be seen from the Sun) of that part of *Venus'* penumbra which traverses the Earth is not a straight line, but part of a large circle. Thus the actual outline on the Earth's surface is not part of a circle. It follows that the parallactic curves in the four quarter spheres ought not to be circular, and the parallactic lines across the other two maps ought to be curved, the direction of their curvature being turned in the direction towards which the shadow is moving for ingress, and the reverse for egress. All the corrections due to this cause are minute, and attain their greatest values at places not suitable as stations either for the application of Delisle's or Halley's method.

A correction has been applied to columns C corresponding to the fact that the maps are severally constructed for a single epoch, while the events to which they relate occupy several minutes.

Table I. *Accelerated Ingress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	[°]	[°]	[°]			^m	^m
Woahoo	22°5	..	19°8	·92	·93	..	11°2
Hawaii	22°3	21°5	19°7	·92	·92	10°3	11°1
Aiton I. (Aleutian)	12°0	..	10°8	·80	·84	..	10°3
Marquesas I.	20°0	23°1	17°7	·71	·66	7°5	7°9
Mouth of Amoor R.	15°0	..	14°0	·57	·62	..	7°6
Jeddo	..	30°9	32°1	..	·50	..	6°8
Otaheite	34°5	34°3	29°7	·59	·54	6°1	6°4
Nertchinsk	..	..	10°1	..	·41	..	5°8
Tsitsikar	..	..	17°0	..	·43	..	5°8
Kirin Oula	..	..	19°5	..	·42	..	5°7
Nagasaki	..	..	32°7	..	·40	..	5°3
Tientsin	..	..	22°2	..	·38	..	5°0
Pekin	..	20°2	20°8	..	·30	3°0	4°3
Shanghai	..	29°5	28°5	..	·25	2°6	3°9
Nankin	..	..	27°1	..	·20	..	3°6
Canton	..	..	35°5	..	·08	..	1°6
Hongkong	..	..	36°2	..	·08	..	1°6

Table II. *Retarded Ingress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	[°]	[°]	[°]			^m	^m
Crozet I.	9°5	..	15°0	·98	·96	..	12°6
Enderby Ld.	..	17°3	20°0	..	·92	10°3	11°8
Kerguelen Ld.	25°0	23°6	27°5	·91	·88	10°3	11°6
Macdonald I.	..	27°2	31°0	..	·85	..	11°2
Kemp I.	..	..	30°0	..	·85	..	11°1
Bourbon I.	4°5	..	12°4	·93	·84	..	11°1
Mauritius	6°0	..	14°1	·92	·81	..	10°7
Amsterdam I.	..	27°6	34°1	..	·77	9°8	10°3
Rodriguez	11°5	..	19°0	·89	·76	..	9°9
Sabrina Ld.	..	..	45°0	..	·73	..	8°2
Adelie Ld.	..	..	45°0	..	·50	..	6°8
S. Victoria Ld.	..	36°4	38°5	..	·46	5°0	6°0
Perth (Aust.)	..	..	65°0	..	·38	..	5°3
Royal Co. I.	..	..	62°0	..	·32	..	4°5
Madras	12°5	..	21°0	·47	·25	..	4°0
Bombay	4°5	..	12°5	·44	·22	..	3°8
Macquarie Ld.	..	..	52°0	..	·25	2°9	3°5
Hobart Town	..	70°0	67°0	..	·20	..	2°8
Adelaide	..	..	75°0	..	·18	..	2°5
Melbourne	..	..	75°0	..	·13	..	2°2

Table III. *Accelerated Egress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Acceleration in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
South Victoria Ld. } (Possession I.) }	..	23'8	25'0	..	'89	10'3	11'4
Adelie Ld.	..	..	34'0	..	'83	..	10'6
Campbell I.	..	..	26'0	..	'79	..	10'3
Emerald I.	..	..	30'0	..	'78	..	10'3
Macquarie I.	24'0	..	32'0	'83	'75	..	9'8
Chatham I.	11'5	..	16'0	'87	'75	..	9'8
Canterbury (N.Z.)	19'0	..	22'5	'81	'71	..	9'3
Wellington	17'0	..	20'0	'81	'70	..	9'2
Sabrina Ld.	..	..	43'0	..	'70	..	9'2
Enderby Ld.	..	41'0	39'0	..	'65	6'9	8'5
Royal Co. I.	..	..	42'0	..	'64	..	8'5
Auckland	15'0	14'4	19'2	'78	'64	8'6	8'5
Kemp I.	..	..	51'0	..	'57	..	7'6
Hobart Town	36'0	36'0	40'0	'66	'57	7'5	7'6
Melbourne	38'0	..	43'0	'58	'48	..	6'6
Sydney	33'0	..	37'2	'59	'48	..	6'6
Adelaide	..	..	47'8	..	'40	..	5'8
Kerguelen Ld.	..	60'5	57'1	..	'40	..	5'0
Crozet I.	..	..	47'5	..	'34	..	4'2
Perth (Aust.)	..	..	66'2	..	'25	..	3'6

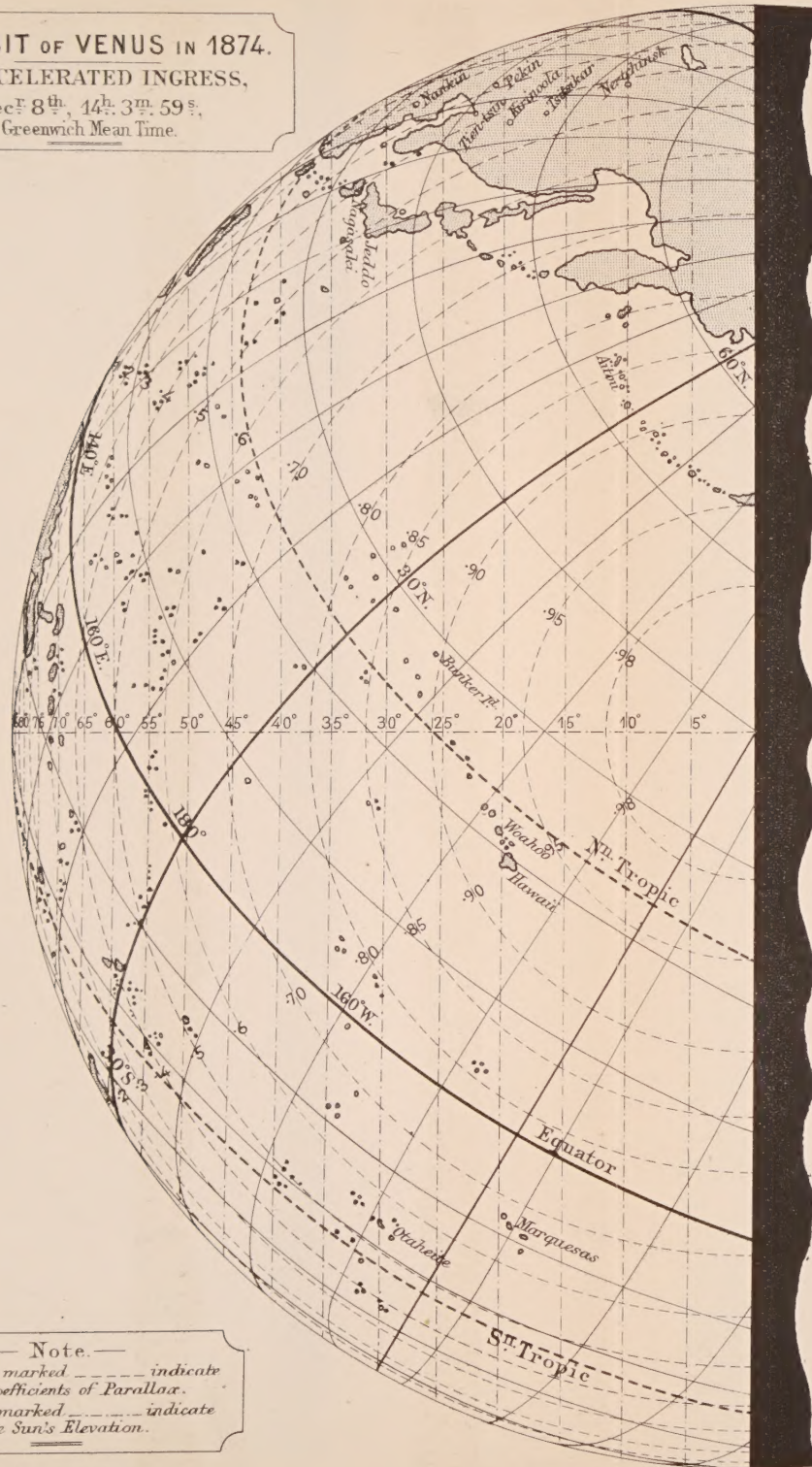
Table IV. *Retarded Egress.*

Station.	Sun's Elevation.			Coefficient of Parallax.		Retardation in Minutes.	
	A	B	C	A	C	B	C
	°	°	°			m	m
Orsk	12'0	..	12'5	'98	'98	..	11'8
Omsk	12'5	..	11'5	'96	'97	..	11'7
Astracan	12'4	..	12'0	'97	'97	..	11'6
Aleppo	14'2	..	14'6	'91	'89	..	10'5
Peshawur	..	..	31'5	..	'85	..	10'3
Alexandria	13'0	..	14'0	'86	'84	..	10'0
Suez	16'0	17'6	16'1	'85	'83	9'4	9'8
Nertchinsk	..	..	10'1	..	'81	..	9'8
Delhi	..	..	38'0	..	'78	..	9'4
Tsitsikar	..	..	12'0	..	'72	..	8'7
Bombay	..	..	45'0	..	'70	..	8'5
Pekin	..	21'2	21'0	..	'70	7'7	8'6
Kirin-Oula	..	..	14'0	..	'69	..	8'4
Tien-tsin	..	..	17'1	..	'68	..	8'4
Calcutta	..	..	45'3	..	'68	..	8'2

TRANSIT OF VENUS IN 1874.

1. ACCELERATED INGRESS,

Dec^r 8th, 14^h. 3^m. 59^s,
Greenwich Mean Time.



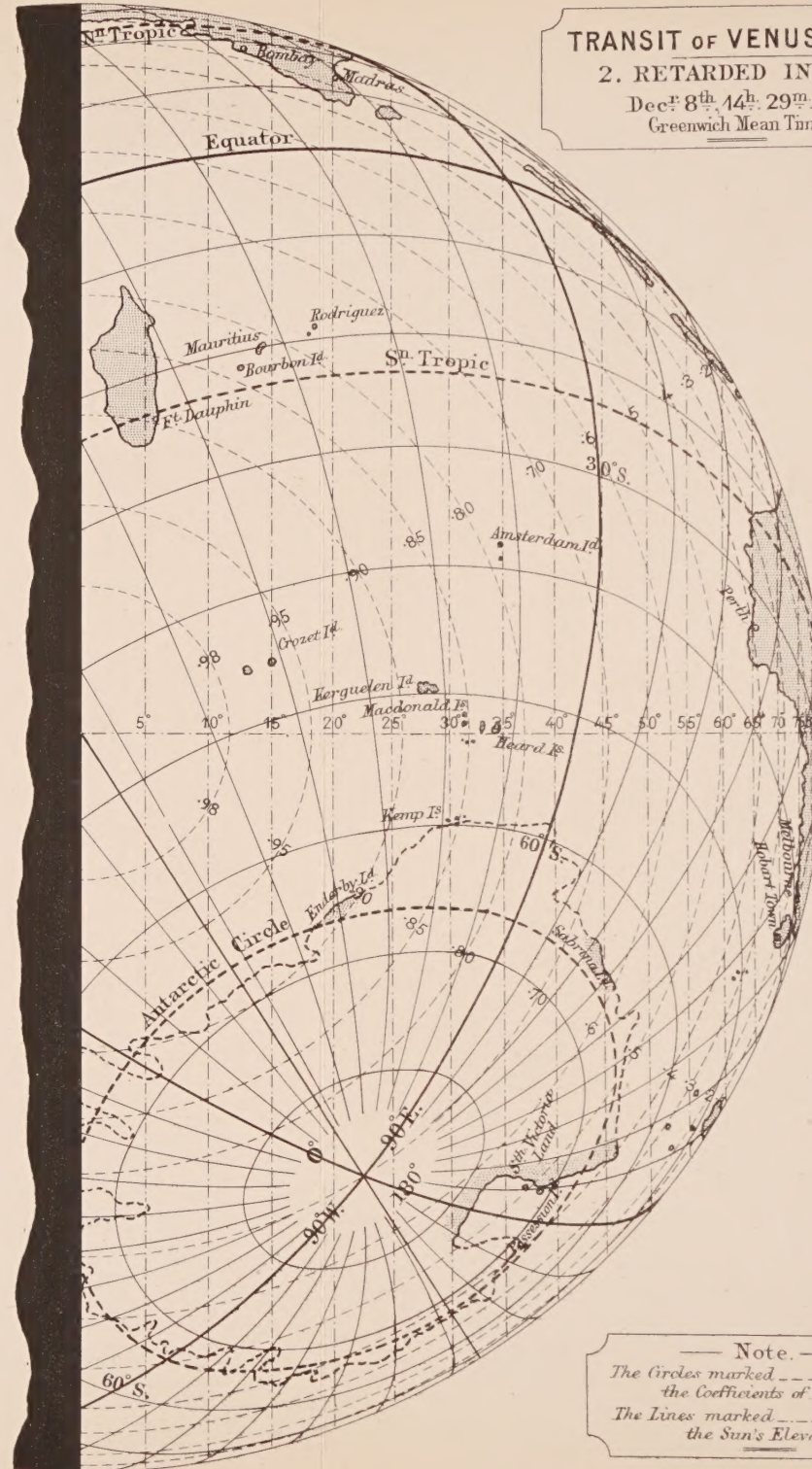
— Note. —

The Circles marked ----- indicate
the Coefficients of Parallax.
The Lines marked ———— indicate
the Sun's Elevation.

TRANSIT OF VENUS IN 1874.

2. RETARDED INGRESS,

Dec^r 8th, 14^h. 29^m. 5^s, •
Greenwich Mean Time.



— Note. —

The Circles marked ----- indicate
the Coefficients of Parallax.
The Lines marked ———— indicate
the Sun's Elevation.

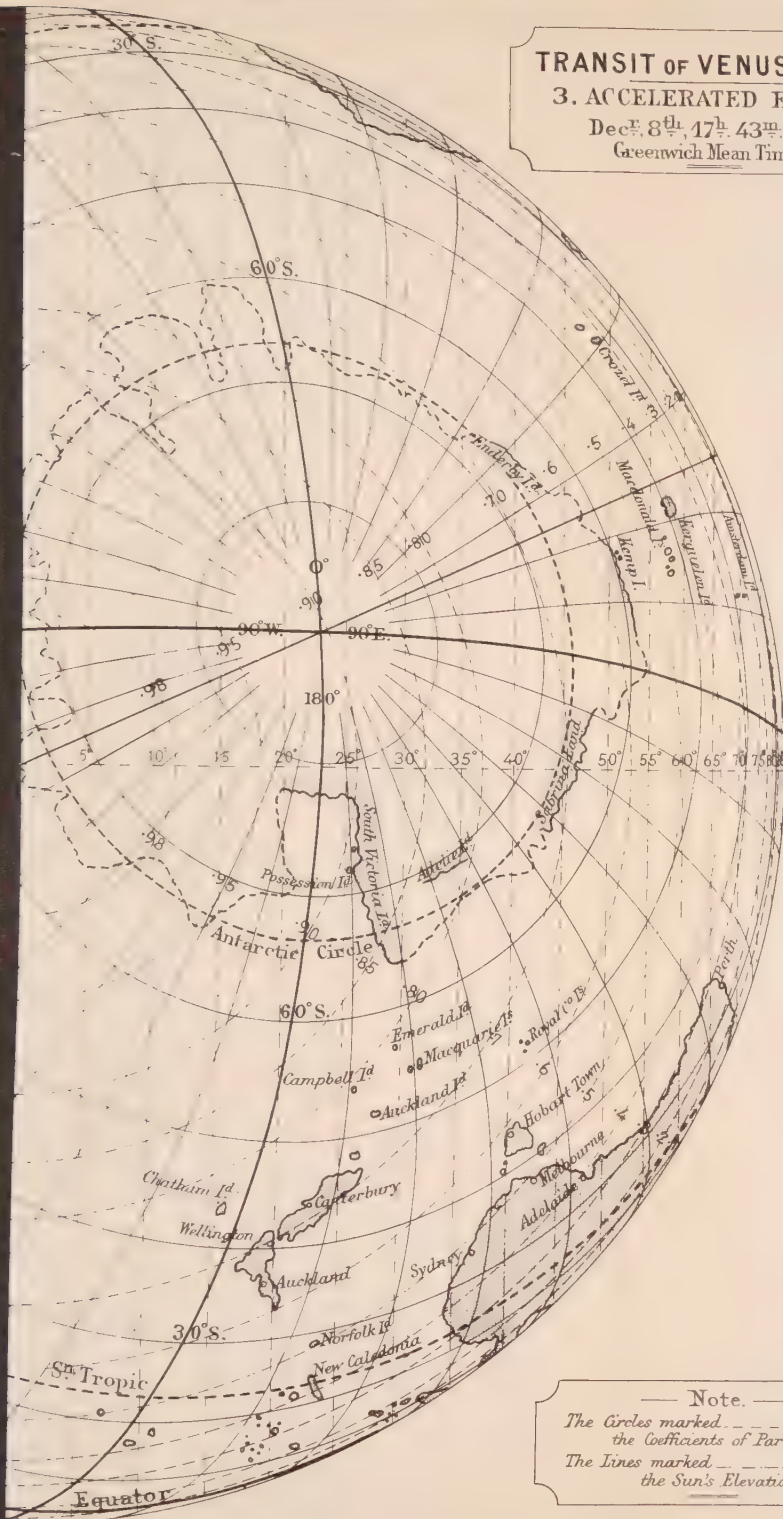


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TRANSIT OF VENUS IN 1874.

3. ACCELERATED EGRESS,

Dec^r. 8th. 17^h. 43^m. 58^s.
Greenwich Mean Time.



— Note. —

The Circles marked — indicate
the Coefficients of Parallax.
The Lines marked — indicate
the Sun's Elevation.

TRANSIT OF VENUS IN 1874.

4. RETARDED EGRESS,

Dec^r. 8th. 18^h. 9^m. 4^s.
Greenwich Mean Time.



— Note. —

The Circles marked — indicate
the Coefficients of Parallax.
The Lines marked — indicate
the Sun's Elevation.

TRANSIT OF VENUS IN 1874.

5. MEAN INGRESS,

Decr. 8th, 14^h. 15^m. 57^s.
Greenwich Mean Time.

Note
The lines marked - - - - - indicate
the Acceleration and Retardation in Minutes.
The Circles marked - - - - - indicate
the Sun's Elevation.



Retarded Ingress

Accelerated Ingress

TRANSIT OF VENUS IN 1874.
6, MEAN EGRESS.
Dec^r 8th, 17^h. 57^m. 5^s.
Greenwich Mean Time.

Dec^r. 8th, 17^h. 57^m. 5^s.

Greenwich Mean Time.

————— Note. —————

*The Lines marked ----- indicate
the Acceleration and Retardation in Minutes.*

*The Circles marked ----- indicate
the Sun's Elevation.*



Station.	Sun's Elevation.			Coefficient of Parallax.		Retardation in Minutes.	
	A	B	C	A	C	B	C
Aden	°	°	36°0	..	·66	m	m
Nankin	..	..	27°0	..	·62	..	7·6
Madras	..	..	52°0	..	·61	..	7·4
Shanghai	..	26°1	26°0	..	·57	6·2	7·2
Canton	..	..	37°0	..	·52	..	6·6
Hongkong	..	..	37°0	..	·50	..	6·5

It will be seen, on a comparison of tables A, B, and C, that the effects of the change of phase are in some cases important. The coefficients of parallax are affected in several instances by more than 0·1, and in two cases by 0·22. In the cases of Crozet Island (Table II.) and Chatham Island (Table III.) solar elevations are so improved, that these stations, which would have to be rejected if central passage were considered, are shown to be well-suited for the observation of internal contacts. The diminution of all the coefficients in Table III., through the change of phase, has an important influence on the value of Delisle's method, so far as egress observations are concerned. It is important to notice, also, that under heads C in Tables III. and IV. many stations not hitherto recognised as available are included among the best places for observing egress. The Indian stations in Table IV. seem too valuable to be neglected. Peshawur is better even than Alexandria; Delhi is not inferior to the latter station (when solar elevation is considered as well as coefficient of parallax). Bombay, Calcutta, and Madras are also excellent. It may be noticed also that Bombay and Madras, which, when considered with reference to central passage, had seemed suitable places for the observation of retarded ingress, are found to have so poor a coefficient of parallax when reference is made to internal contacts, that it would be useless to observe ingress there (so far at least as the application of Delisle's method is concerned).

Of course, it will be impracticable for this country to send observers to more than a certain number of stations. But it is not unlikely that besides Russia, France, and England (the only countries specially concerned in the transit of 1874), other nations may care to take part in the solution of the noble problem of determining the Sun's distance; and thus it seems advisable that all the stations where there will be any chance of obtaining useful observations, should be tabulated as nearly as possible according to their relative values.*

* It may not be amiss to point out that the numbers tabulated under head C in the above tables may be readily tested in a few minutes for any assigned station. Indeed, the whole work of determining the circumstances of the transit for internal contacts, as seen from any station, may be gone through in about an hour, and the main part of the work thus gone through would not require to be repeated for other stations. Although I have no reason to doubt the accuracy of the results tabulated above, it may be well that they should be checked.

As regards the comparison between Delisle's and Halley's method for the transit of 1874, I may remark that M. Puiseux's results seem to have been somewhat misinterpreted. He does not anywhere speak of Halley's method as the best, but simply states that he can see no reason why it should not be applied: nor do his figures establish the superiority of Halley's method.

I believe I shall be able to show, however, that there is at least a possibility that Halley's method may be so applied to the transit of 1874, as to give absolutely the best means of deter-

The justice of the following processes will be obvious to every one acquainted with the principles on which the transits of an inferior planet depend:—

Take out from the *Nautical Almanac* and tables of the planetary elements,—

App. Semi-diameter of Sun on December 8	..	= D
„ „ Venus in inf. conj.	..	= V
Horizontal Parallax of Venus (in inf. conj.) reld. to Sun		= P
Sun's Southern Declination on December 8	..	= δ
Equation of Time		= ϵ

On any convenient scale describe concentric circles with radii

$$\begin{array}{lll} \text{(i.) } D & \text{(ii.) } D + V & \text{(iii.) } D - V \\ \text{(iv.) } D - V + P & \text{and} & \text{(v.) } D - V - P \end{array}$$

On circle (ii.) take (according to Mr. Hind's elements of the transit) position-angles $130^{\circ} \cdot 7$ from South towards East, and 160° from South towards West, and join the points thus determined. Determine with a protractor the position-angles Q_1, Q_2, Q_3 , and Q_4 , for the intersections of this chord with the circles (iv.) and (v.). Again, making the extremities of the chord correspond to hours $13^h 47^m$ and $18^h 26^m$ (from Mr. Hind's elements of the transit) divide the chord into hour-divisions. Thence determine epochs H_1, H_2, H_3, H_4 , corresponding to the intersections of the chord with the circles (iv.) and (v.).

Now adjust a globe so that, looked at from above (vertically), its appearance shall be that presented by the earth, supposed to be seen from the Sun, at apparent solar time $H_1 + \epsilon$, and with southerly solar declination δ . From the southern intersection of the brazen meridian with the horizon-circle, take an arc of Q_2 towards the East along the latter circle. This will give the place where ingress is most accelerated. To determine the value of any station near this point, measure the arc-distance of the station from the point (α), and from the horizon-circle (β). Then $\cos \alpha =$ coefficient of parallax, and $\beta =$ Sun's elevation. And thus the coefficients and solar parallax may be determined for any number of stations, with corresponding methods for retarded ingress and for egress.

It may be necessary to notice that, to convert coefficients of parallax into minutes of acceleration or retardation, reference must be made to the intervals of maximum acceleration and retardation. These are, for accelerated ingress and retarded egress (Tables I. and IV. respectively) $11^m 58^s$; for retarded ingress and accelerated egress (Tables II. and III. respectively) $13^m 8^s$. A coefficient of parallax p corresponds for the former pair of cases to a time-interval

$$[11^m 58^s - 35^s (1 - p)] p,$$

for the latter to a time-interval

$$[13^m 8^s + 35^s (1 - p)] p.$$

mining the Sun's distance available before the transit of 2004 (about the circumstances of which I know nothing). It only requires that the same energy should be devoted (either by England or some other nation) to the coming transit, which has been called for in relation to the transit of 1882. There will also be the same chance of failure in one case as in the other.

To prove the justice of these views I point to Nertchinsk (or its neighbourhood) as a suitable station for observing the lengthened transit, and the neighbourhood of Enderby Land as a proper station for observing the shortened transit. The above Tables give for the former station a lengthening of ($5^m.7 + 9^m.8$) or $15^m.5$; and for the latter a shortening of ($11^m.8 + 8^m.5$) or $20^m.3$. The total difference of duration is thus shown to be $35^m.8$. Setting against this the best case for Delisle's method (Woahoo from Table I. and Crozet Island from Table II.), we get the total difference of absolute time, is ($11^m.2 + 12^m.6$) or $23^m.8$. And the relative values of the methods (in these cases the most favourable for each) are given by the formula

$$\frac{\text{Halley's}}{\text{Delisle's}} = \frac{(35.8)^2}{(23.8)^2} \frac{\{(4.28)^2 + (1)^2\}^*}{(4.28)^2}$$

$$= \frac{2477^\dagger}{2075} \text{ approximately;}$$

and in this ratio, independently of all the other advantages which it presents, does Halley's method surpass Delisle's. By taking Kemp Island, Adelie Land, Victoria Land, Crozet Island, or Kerguelen's Land for the southern, and Tsitsikar, Kirin-Oula, or Tien-tsin, for the northern stations, we get differences of duration ranging from 33^m to $30^m.6$, corresponding (according to Mr. Stone's formula) to a range from 24^m to $22^m.2$, in the case of Delisle's method. There is little chance of the latter method being applicable with a difference greater than the lowest of these methods; and the highest is a difference which the best stations will not give by Delisle's method.

When it is remembered that Halley's method is so much the simpler, and that stations selected with reference to it give a double chance of at least a useful observation, the above considerations seem to decide the question of the relative values of the two methods in 1874.

* Here I have used the values given in Mr. Stone's paper (*Monthly Notices*, April 8, pp. 251, 252). He would have obtained a larger value for e had he calculated for internal contacts. This would, of course, have been a change unfavourable to my case. But against this I set the fact that his careful examination of the circumstances of internal contacts will tend to render the probable errors much smaller (relatively) in 1874 than they were in 1769, when the observers had very vague notions of the phenomena they were to expect and to watch.

† It would not be correct to reduce the number of minutes 35.8 in a certain proportion, and take the excess over $23^m.8$, as measuring the superiority of Halley's mode. The proper way of treating the question is to indicate the relation between the two modes by a ratio, as above.

Lastly, as to the relative values of the transits of 1874 and 1882, considered with reference to Halley's method.

Mr. Stone's remark, that the choice of stations is limited to those at which the Sun will have an elevation of at least 10° , reduces the maximum available difference of duration in 1874 from $36\frac{3}{4}^m$ (the value I had before mentioned) to $35\frac{3}{4}^{in}$. But the transit of 1882 is much more seriously affected. The suggested station near Sabrina Land must be rejected at once. And, although the suggested station near South Victoria Land corresponds to an elevation of 10° at ingress (see map 5, *Monthly Notices* for December last), there is no accessible spot in that neighbourhood which will give any such elevation. At Possession Id. the Sun's elevation will not be much more than 5° at ingress; and at Coulman Island, the most southerly station which Antarctic seamen hope to reach, the Sun's elevation will be but 7° ; and even if these islands were suitable, they give a difference of duration perceptibly less than that which I had dealt with in my former paper.

I must add that I had fully taken into account the difference in the clinging of the disk of *Venus* to the Sun's limb in 1874 and 1882. Indeed, I had adopted a considerably greater proportion than that indicated by Mr. Stone. But, as the principle he lays down requires that both Sabrina Ld. and Victoria Ld. should be dismissed from our consideration in 1882,—and as there is absolutely no other southern station at all comparable with these two, as far as the lengthening of the transit's duration is concerned, we seem forced to the conclusion that if Halley's method fails totally for either of the coming transits, it is for that of 1882 and not for that of 1874.

On the general question of the value of the transit of 1874, I find myself compelled to adopt a different opinion from that which Mr. Stone has expressed. Although the comparative slowness of ingress and egress increases the probable error, it increases the available time-differences in at least the same proportion,—I say, *at least*, because I cannot believe that the errors of an observation of ingress or egress increase fully in the proportion of the slowness with which the phenomena succeed each other. But even assuming this unfavourable view, it may yet be shown that, *cæteris paribus*, slow transits (*i. e.* transits of short chord) are as valuable as more rapid ones when Halley's method is considered, and more valuable when Delisle's method is considered. For, in the first case, we have

- (1), Value of a transit varies as maximum observable difference;
- (2), Maximum observable difference varies as slowness of rate of ingress or egress* (*cæteris paribus*);
- (3), Value of a transit varies as
$$\frac{1}{\text{Slowness of rate of ingress}};$$

* Estimated, of course, perpendicularly to the limb.

therefore, for transits of different rates, but having circumstances otherwise equal, the value is appreciably constant, so long as the chord of transit is not too near a diameter of the Sun.

For the second case we have (1) and (2) unaltered, but in place of (3) we get the

$$\text{Relative value of transit varies as } \frac{1}{\left(\frac{k^2}{v^2}\right) + l^2} ;$$

where k is some constant, v represents the rate of ingress or egress, and l is the error in determining longitude of a station.

Thus, the value of a transit varies as $\frac{1}{v^2} \div \left(\frac{k^2}{v^2} + l^2\right)$ or inversely as $(k^2 + v^2 l^2)$, and the slower transits are therefore the most favourable.

It is worthy of notice, however, that, *as a rule*, when there are two transits separated by eight years, the first is the less favourable for the application of Halley's method. The present case shows that to this rule there are exceptions.

Field Notes on the Zenith Telescope. By G. Davidson, Esq.

(Communicated by Prof. Piazzì Smyth. Abstract.)

The principle of the method of determining the latitude with the Zenith Telescope is based upon the proposition that when the meridional zenith distances of the two stars at their upper culmination (one being north and one south of the zenith of a station) are equal, the colatitude is the mean of their north polar distances.

When the zenith distances of the pair of stars differ by an arc less than that subtended in the field of view of the telescope, that arc is measured by means of the micrometer attached to the telescope; and, at the times of observation, the error in direction of the vertical axis of the instrument in the plane of the meridian is measured by a delicate level.

The micrometer and level are therefore essential means by which the principle of the method is reduced to a practical field application: whilst the generally unsatisfactory correction for refraction in observations depending on the measure of simple zenith distances is avoided by applying the differences of refraction for the small difference of zenith distance of the two stars.

The paper contains a full discussion (illustrated by examples) of the methods of ascertaining the instrumental errors and other corrections as used in the U. S. Coast Survey; and of two different methods used for reducing the observations; and it is shown how the instrument may be used for extra-meridian observations for latitude, &c., which observations, by means of known stars too far apart for the application of the preceding accurate methods, may (it is remarked) be often used with advantage on a geographical reconnaissance or for some special object.

Note on Lambert's Theorem for Elliptic Motion.

By Prof. Cayley.

Consider any two positions, A, B, in an elliptic orbit, focus S, and semi-axis major = a ; then if ϵ, ϵ', c denote the radius vectors SA, SB and the chord AB respectively, and if $P, = \frac{2\pi a^{\frac{3}{2}}}{\sqrt{\mu}}$, be the periodic time, the time of passage from A to B is given by the formula

$$\text{Time A B} = \frac{P}{2\pi} (\chi - \chi' - \sin \chi + \sin \chi')$$

where

$$2a \cos \chi = 2a - \epsilon - \epsilon' - c, \quad 2a \cos \chi' = 2a - \epsilon - \epsilon' + c.$$

To fix the ideas we may consider the time of passage as being in every case positive; and, for Time A B, the motion from A as being towards the apocentre; Time B A will, of course, in like manner denote that the motion from B is towards the apocentre; and we thus have according to the positions of A, B, either Time A B = Time B A; or else Time A B + Time B A = P.

This being so (see the *Theoria Motus*, p. 120), χ will be always a positive arc between 0 and 360° ; χ' a positive or negative arc between 0 and $\pm 180^\circ$; and moreover χ' will be positive or negative according as the described focal angle is $< 180^\circ$ or $> 180^\circ$; whence, $\cos \chi'$ being known, the arc χ' is determined without ambiguity.

But as noticed in the place referred to, there is when only $\epsilon, \epsilon', c, a$, are known, a real ambiguity as regards the arc χ ; viz. χ may be either the arc $> 180^\circ$ or the arc $< 180^\circ$, having for its cosine the given value of $\cos \chi$. For, given the points S, A, B, and the semi-axis major a , there exist two elliptic orbits determined by these data; and the two values of χ correspond to the times of passage between A and B, in these two orbits respectively. If, however, the actual orbit be given, there is no longer any real ambiguity; and it must be possible to decide between the two values of χ : the criterion is, in fact, a very simple one, viz. drawing a chord from A through the other focus H of the ellipse, this either separates, or it does not separate, B from the force-focus S; and I say that in the expression of Time A B, in the former case (viz. when chord A is a separator) we have $\chi < 180^\circ$; in the latter case (viz. when chord A is not a separator) we have $\chi > 180^\circ$.

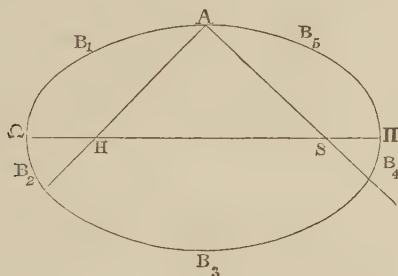
It of course follows that, in the case of transition, when the line A B passes through H, we must have $\chi = 180^\circ$: this is at once seen to be so; for $\chi = 180^\circ$ gives the condition $4a = \epsilon + \epsilon' + c$; but if σ, σ' , are the distances of A B from the focus H, then $2a = \epsilon + \sigma, 2a = \epsilon' + \sigma'$, and the condition becomes $\sigma + \sigma' = c$; that is AB must pass through H.

As a verification of the new criterion, I consider the point A as having a fixed position on the orbit, but the point B as having successively different positions; and writing down the two formulæ

$$\text{Time A B} = \chi - \chi' - \sin \chi + \sin \chi',$$

$$\text{Time B A} = \omega - \omega' - \sin \omega + \sin \omega',$$

(where for simplicity the constant factor $P \div 2 \pi$ is omitted) I proceed to compare these for different positions of the point B. We have, in every case, $\cos \omega = \cos \chi$, and $\cos \omega' = \cos \chi'$; whence (χ, ω being each positive and less than 360°) $\omega = \chi$ or else $\omega + \chi = 360^\circ$, viz. the former equation subsists if ω, χ , are each less or each greater than 180° , the latter if the one is greater, the other less than 180° . And again (χ', ω' being each less than $\pm 180^\circ$) we have $\omega' = \chi'$, or else $\omega' = -\chi'$, according as ω', χ' have the same or opposite signs.



Now in the figure, suppose that B occupies successively the different positions $B_1, B_2 \dots B_5$, the criteria for χ, χ' (or ω, ω') give as follows,

Ch. A.	Ch. B.	$\therefore$		$\angle A S$	$\angle B S A$.		
sep.	not	$\chi < 180^\circ$	$\omega > 180^\circ$ or $\omega + \chi = 2 \pi$	$< 180^\circ$	$> 180^\circ$	$\chi' = +$	$\omega' = -$ or $\omega' = -\chi'$
sep.	sep.	$<$	$<$	$<$	$<$	$+$	$+$ „ $\omega' = \chi'$
not	not	$>$	$>$	$>$	$>$	$+$	$+$ „ $\omega' = \chi'$
not	not	$>$	$>$	$>$	$>$	$-$	$-$ „ $\omega' = \chi'$
not	sep.	$>$	$<$	$>$	$<$	$-$	$-$ „ $\omega' = -\chi'$

Hence substituting for ω, ω' their values in terms of χ, χ' , we have

$$\begin{aligned} \text{Time A B}_1 \dots B_5 &= \chi - \chi' - \sin \chi + \sin \chi' \\ \text{„ B A}_1 &= 2 \pi - \chi + \chi' + \sin \chi - \sin \chi' \\ \text{„ B A}_2 &= \chi - \chi' - \sin \chi + \sin \chi' \\ \text{„ B A}_3 &= \chi - \chi' - \sin \chi + \sin \chi' \\ \text{„ B A}_4 &= \chi - \chi' - \sin \chi + \sin \chi' \\ \text{„ B A}_5 &= 2 \pi - \chi + \chi' + \sin \chi - \sin \chi' \end{aligned}$$

and thence (restoring the omitted factor $P \div 2\pi$)

$$\begin{array}{rcl} \text{Time } A B_1 & + & \text{Time } B A_1 = P, \\ \text{,, } A B_2 & - & \text{,, } B A_2 = 0, \\ \text{,, } A B_3 & - & \text{,, } B A_3 = 0, \\ \text{,, } A B_4 & - & \text{,, } B A_4 = 0, \\ \text{,, } A B_5 & + & \text{,, } B A_5 = P, \end{array}$$

which are the relations which in fact subsist between the times $A B_1$ and $B A_1$ &c.

On the Nature of the Appearances on Sun-spots, called Bridges of Light. By Professor E. W. Brayley.

At the last meeting of the Society I suggested as desirable an inquiry whether some of the appearances, termed *Bridges*, of which the drawings, exhibited by Mr. Bidder and Mr. Browning, of the Sun-spots observed and described by them,* presented remarkable instances, were not, in reality *faculæ* over the spots—of the nature of the floating *faculæ* described and photographed by Mr. De La Rue—viewed in a direction perpendicular to the Sun's surface, or in that of the geometrical axis of a spot.

My opinion that such would be found to be the fact eventually was supported by a statement made by Mr. Huggins, that he had observed *Bridges* exceeding in brightness the average brightness of the photosphere.†

I have since found that in my third paper on the Physical Constitution of the Sun, in the *Companion to the Almanac* (for 1866, p. 77) the following observations on the phenomena in question are cited from the *Solar Physics* of the Kew observers; being a portion of their inductive argument that a spot is a phenomenon which takes place beneath the level of the brighter part of the Sun's photosphere.

“There are a good many instances in which a spot breaks up in the following manner. A bridge of luminous matter of the same apparent luminosity as the surrounding photosphere, and unaccompanied by any penumbra, appears to cross over the umbra, or centre, of a spot. There is good reason to think that this bridge is really above the spot; for were the umbra an opaque cloud, and the penumbra a semi-opaque cloud, both being above the Sun's photosphere [as supposed by Kirchhoff], it is unlikely that the spot would break up in such a manner that the terrestrial observer should not perceive some penumbra accompanying the luminosity. Again, detached portions of luminous

* *Monthly Notices* for May 14, pp. 300–302.

† See *Astronomical Register* for June, 1869, pp. 124, 125.

matter appear to move across a spot without producing any permanent alteration.”*

These observations are in entire harmony with my opinion that these bridges are portions of faculæ, and with the confirmatory observations of Mr. Huggins and Mr. Browning, of the great brightness of some bridges.

It is requisite that I should here remark that I confine this interpretation to the phenomena for which I adopt the original designation by Sir J. F. W. Herschel, of “*bridges of light*;”† not extending it to the other “bridges” seen to connect the opposite sides of a spot, the constitution of which is manifestly the same with that of the penumbra, and which have every appearance of being at the same level with them. This discrimination is in agreement with the inferences of the Kew observers already stated.

From the structure of the two bridges on the spot observed by Mr. Browning on March 14, which, as described by him, was evidently the characteristic structure of penumbra, while yet the bridges, it is said, were the brightest parts of the object represented, it is probable that in his observation the two kinds of bridges were optically confounded together, on account of the partial superposition, in this instance, of bridges of light over penumbral bridges.‡

* *Solar Physics*, First Series, p. 31, sect. 27. In a note on the fact last described I have suggested that these detached portions of luminous matter are, perhaps, some of the hypothetical bodies which I have termed *meteoritic masses*. After considering, however, the observations, both telescopic and spectroscopic, since made of the solar disk, I no longer retain this idea.

The term “*meteoritic masses*” is nearly equivalent to that of *meteoroids*, used by Professor H. A. Newton to designate the bodies which become sensible to us as meteors, but which, until recently, were otherwise utterly unknown, though their existence was certain, and had been the subject of several reasonable hypotheses. Now, however, we have grounds for believing, according to the judgment of astronomers in general, that they are related to comets, and, in my own opinion, that they have also a genetic relation to the Sun. See *Companion* for 1866, pp. 91–95, and for 1868, pp. 125–126; also, *Proceedings of the Royal Society* (vol. xiv. pp. 120–129) for March, 1865, and *Report of the British Association for the Advancement of Science* for that year, Appendix to Report of Committee on Luminous Meteors, pp. 130–131, and 140–142.

† See *Results of Astronomical Observations made at the Cape of Good Hope*; “*Observations of the Solar Spots*,” p. 435 (par. 428). The accompanying plate (xvii.) presents, on a small scale, in figs. 4, 6, 7, 13, characteristic curved, if not spiral examples, of these objects.

‡ Graphical representations of celestial objects should, of course, describe with all possible fidelity what the observer believes that he sees, leaving the due interpretation of the several appearances which make up the visible projection for future scrutiny. But it would appear to be desirable that telescopic observers of the Sun should now endeavour to discriminate between the two kinds of “bridges,” and to ascertain the respective limits of each, where they are not superposed. In many figures of solar configurations both appear to be represented, but as if they were continuous portions of the same physical objects, differing only in brightness.

In observing the Sun in the profound manner, so to speak, which, in the actual condition of heliography and solar physics, astronomers are called upon to adopt, “the combination of the photographic record” of the telescopic pro-

The varying forms of the bridges of light, observed—always within the penumbral area, and commonly at its inner edge bordering the umbra, but sometimes crossing the latter,—on the remarkable spot seen in October, 1865, as described and figured by the Rev. F. Howlett,* are also strictly conformable with the view of their nature now taken. They are such as would be presented by the extended vertices of a spiral or curved sheet or volume of flame or incandescent gaseous matter, rising from the area of the umbra. We see in them, in all probability, the upper termination (as a physical object, neglecting at present the consideration of its chemical nature) of the vorticose or cyclonic torrent, of which, as viewed in horizontal section, the umbra of the spot is merely the visual projection. In all probability, again, such torrents are liquid below and aeriform above, a constitution which would account for some apparent inconsistencies in the recorded spectroscopical observations of the several physical elements of spots. The spectra of bright lines seen over umbræ, both by Mr. Huggins and Mr. Lockyer, would naturally be caused by their upper gaseous portions; a subject to which I have alluded in the *Monthly Notices* for January.

I intended now to discuss, as then intimated, the mutual relations of Sun-spots, faculæ, and prominences, considered as the results of one continued action, beginning in the nucleus of the Sun, including the compatibility of that induction with the local distribution of the spots; and also the character of the markings by which the faculæ-prominences may be expected to be recognised in photograms of the solar surface viewed in the stereoscope. But I must reserve both subjects for the next session of the Society.

I may remark, however, that the recent spectroscopical observations of Mr. Lockyer† and Padre Secchi,‡ assuming their accuracy, are conclusive as to the identity of faculæ and prominences as portions of the same physical objects, while they are in perfect accordance, also, with the theory of the constitution of

jection with “the stereoscopic examination of it” will be invaluable. I formerly remarked that, “Admitting the powers of” this method of research “to be correctly estimated by Mr. De La Rue, and assuming that there is no source of fallacy in its use, it may be doubted whether any other means at present known to science will so adequately avail for the discovery of the order of superposition of the several parts or objects of which a given area of the visible photosphere consists, or their arrangement in altitude vertical to the Sun’s surface.”—*Companion*, 1864, p. 40.

Mr. De La Rue’s observations of the floating faculæ, which I have discussed in the *Monthly Notices* for January, and those of the nearly related phenomenon of the comparative elevation of bridges of light, cited from him and the other Kew observers in the present paper, are accordant examples of the value of this combined method in astronomical research, a crucial addition to which is now afforded by the spectroscope.

* *Monthly Notices*, November, 1865, pp. 12–20, figs. 5 to 10.

† *Proceedings of the Royal Society* (vol. xvii. pp. 415–418) for April 29, 1869.

‡ *Comptes Rendus* (vol. lxxviii.), May 10, 1869, p. 1082. The entire contents, however, of P. Secchi’s two papers here published bear immediately on the objects of this reference:

the spots which I announced in a paper, entitled "Inferences and Suggestions in Cosmical and Geological Philosophy," inserted in the *Proceedings of the Royal Society* for March, 1865. This would require that the spectrum of the umbra of a spot should not exhibit any line of absorption which is not present in that of the Sun's normal surface, and also that no ordinary solar line should be wanting in it; facts actually observed by Mr. Huggins;* with whose results, in this respect, those obtained by his contemporaries are in agreement.

London Institution, June 10, 1869.

On Solar Currents. By C. H. Weston, Esq.

(Extracts from Note-Book.)

1869, June 4th. Large spot changed since the 2nd. Penumbra with increased margin, especially on S. shore of umbra.—
5th June. A large luminiferous promontory running out from E. side of S. shore with extremity expanded. Shut off Sun's disk by diaphragms. Now the expanded or club end was seen bifurcated, and of the two branches the W. one longest. In afternoon W. branch more elongated, and also with branchlet at right angles to the branch. Towards end of branchlet two little bud-dings like incipient transverse arms of a Roman cross.

One end of this luminous promontory was attached to the penumbra, and not to the overlying photosphere. The margin of the penumbra was so broad on the S.E. side that not only was the connexion apparent, but the penumbra itself visibly intervening between the penumbral promontory and photosphere. Thus, while the S.E. extremity was undoubtedly penumbral (physically and chromatically) the other was as brilliant as the photosphere itself and the terminal point especially so; preponderating evidence that the N.W. end was in the act of being lifted up into the upper regions of the photosphere.

6th June. Same branch and branchlet increased in breadth and now pretty uniformly broad, and reaching to W. margin. Perceived well by strong contrast of subjacent umbra, that the promontory was unquestionably *arched* on the S.E. side, while the N.W. end appeared to touch the photosphere and partook of the same brilliancy; further confirming the idea that the part of S.E. penumbra was arched up (from rising currents) with its N.W. end on the *higher* photospheric elevation.

7th June. Examined another spot. No penumbra apparent on the S.E. side. Faculous matter (clearly part of the photosphere)

* See *Phil. Trans.* 1868, p. 553.

extended across the umbra. On N.W. side the penumbra was visible on each side of the jutting cloud. Towards N.W. the extremity was dichotomized by a black line (recalling *Saturn's* ring) evidently showing umbra beneath. Three hours later the cloud lengthened, but throughout the entire length same lustre at photosphere, showing an approximate horizontal movement from S.E. to N.W.

Another spot examined, showing a bridge truly penumbral, extending from E. to W. margin of umbra. In the afternoon the bridge more brilliant, and so also the inner margin of the penumbra (in contact with the umbra), while the external margin was not so illuminated,—evidence of upward movement both in the bridge and peripheral parts of penumbra bordering on the umbra.

These three cases seem to show locally-upward and horizontal currents.*

I have given these descriptions as concisely as possible, and will not, therefore, add any remarks on luminosity. But I must confess that I concur with M. Faye in thinking there is no necessary connexion between the degree of photospheric luminosity and solar caloric.

*Ensleigh Observatory,
Lansdowne Bath, June 1869.*

*Probable Errors of Greenwich Observations in Zenith Distance,
estimated by mere discordances from the separate means. By
E. J. Stone, Esq.*

No.	Z. D.	P. E.	No.	Z. D.	P. E.
1	0°	0'47	14	44°	0'59
2	26	0'48	15	45	0'60
3	28	0'49	16	46	0'61
4	30	0'50	17	47	0'62
5	32	0'51	18	48	0'63
6	34	0'52	19	49	0'64
7	36	0'53	20	50	0'65
8	38	0'54	21	51	0'66
9	39	0'54	22	52	0'67
10	40	0'55	23	53	0'68
11	41	0'56	24	54	0'69
12	42	0'57	25	55	0'70
13	43	0'58	26	56	0'71

* Used (full apertures) two Newtonian Equatoreals, 9'0 and 14'15 inches, and 8'75 and 16'5 feet respectively.

No.	Z. D.	P. E.	No.	Z. D.	P. E.
	°	"		°	"
27	57	0·72	42	72	0·87
28	58	0·73	43	73	0·89
29	59	0·74	44	74	0·92
30	60	0·75	45	75	0·95
31	61	0·76	46	76	0·98
32	62	0·77	47	77	1·02
33	63	0·78	48	78	1·08
34	64	0·79	49	79	1·14
35	65	0·80	50	80	1·20
36	66	0·81	51	81	1·28
37	67	0·82	52	82	1·38
38	68	0·83	53	83	1·52
39	69	0·84	54	84	1·68
40	70	0·85	55	85	1·92
41	71	0·86	56	86	2·29
			57	87	3·32

The number of observations employed in obtaining these probable errors is more than 2000.

The probable errors were first determined for ten principal points, and a curve was traced amongst these points: the results given are those read off from the curve. Observations of *Sirius* and *Rigel*, which are incessantly observed at Greenwich, and observations of which are often made under unfavourable circumstances and in the daytime, have rather larger probable errors than the general average, and have not been included in the results.

Occultations of Stars by the Moon. By John Joynson, Esq.

I have not had an earlier opportunity of seeing Mr. Plummer's paper remarking on one of mine on "Occultations of Stars by the Moon," in vol. xxix. No. 5.

I am afraid that I have given Mr. Plummer a considerable amount of trouble; but my apology, if he considers one needful, must be that I found that sometimes, when the disappearances and reappearances occurred at or about the same angle, there was a greater difference than seemed to be accounted for by the parallax.

I am glad, however, to find that I was mistaken, and that the calculated times agreed so well with the observations I sent to the Society, with the exception of that of the reappearance of *120 Tauri*.

Occultations of Stars by the Moon.

			G.M.T.			
			h	m	s	
1869.						
19 Feb.	48 Tauri	Disappearance	9	4	40.3	cloudy at reappearance.
23	ζ' Cancri	„	10	11	40.2	„
24	π^2 Cancri	„	10	45	16.9	
		Reappearance	11	43	3.5	
22 March	γ Geminorum	Disappearance	9	17	16.7	
		Reappearance	10	26	21.8	
16 April	119 Tauri	Disappearance	7	29	44.2	
		Reappearance	8	23	19.8	
	120 Tauri	Disappearance	8	18	25.6	
		Reappearance	8	52	48.5	
18 May	Regulus	Disappearance	9	54	27.9	
		Reappearance	10	7	25.2	
Latitude			53° 28' 24" N.			
Longitude			0 ^h 12 ^m 7 ^s W.			

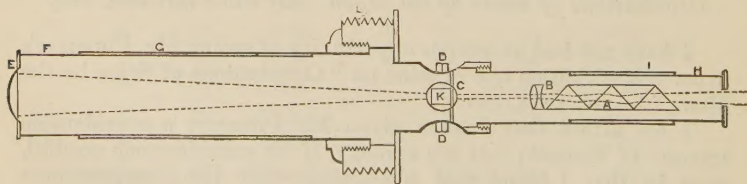
Waterloo, near Liverpool,
3 June, 1869.

On a Simple Form of Star Spectroscope.

By John Browning, F.R.A.S.

A simple, efficient, and economical spectroscope for Astronomical work has long seemed to be a desideratum.

The instrument which I have the honour to exhibit and describe to the Society will, I think, be found better adapted for use with telescopes of moderate aperture than any other contrivance hitherto proposed.



In the diagram, A is a compound direct-vision prism consisting of five prisms, B is an achromatic lens which focusses on the slit C, by means of a sliding-tube H, both the prisms and lens are fastened in this tube; K is a small right-angled prism covering half the slit, by the aid of which light may be seen reflected through the circular aperture in front of it.

In this manner a comparison may be made with the spectra of metals or gases. The reflecting prism, with the ring to which it is attached, can be instantly removed and the whole length of the slit used if desired. DD is a ring milled on the edge, on turning this round both edges of the slit are made to recede from each other equally, being acted on by two hollow excentrics. The lines can thus be increased in breadth without their centres being changed, a point of importance. E is a cylindrical lens attached to the tube F, which slides in another tube G.

To use the spectroscope the adapter L, which is the same size as the Society's thread, has only to be screwed into the eyepiece of the telescope in the place of the ordinary Huyghenian eye-piece.

The drawer must then be adjusted so that the slit G comes exactly to the focus of the object-glass. This should be tried beforehand, by getting an image of the Sun and then making a mark on the drawer-tube of the telescope. When this has been done the tube can be set by this mark, and the spectroscope screwed into it at any time without any trouble in adjustment.

If the cylindrical lens be removed the spectrum of a star will be a mere line of light.

The cylindrical lens is for the purpose of widening this line to such an extent that the lines in the spectrum may readily be discerned. For this purpose the lens must be placed *with its axis at right angles with the slit*, and the best distance from the slit will be between three and six inches. The nearer it is brought to the slit the broader will be the spectrum, but it should not be used too close, on account of the diminution of the light.

When it is desired to obtain the spectra of planets, comets, or nebulae, or indeed any heavenly bodies possessing considerable diameter in the telescope, the cylindrical lens may be dispensed with advantageously.

The weight of the instrument, as I have figured and described it, being only about seven ounces, much less than an ordinary spectrometer, it will not seriously affect the balance of a telescope.

Various portions of this apparatus have been for some time used by different observers. The arrangement of the whole in combination with the new slit is the only novelty in the contrivance.

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